netism (and the ultimate theory of magnetism is undoubtedly kinetic)[1] involves quite a different class of motions from those to which the beautiful phenomena discovered by Mr. Guthrie are due. He rather wished to point out the close connection that existed between the laws of some of these actions and the laws of magnetism, which, while involving some remarkable coincidences, involves certain contrasts decisive against any hypothesis, such as the ingenious one[2] of Euler, explaining magnetism by fluid motion directly comparable with that which forms the subject of the present communication." KELVIN.

The University, Glasgow, February 13.

The Stress in Magnetised Iron.

I AM glad to see from Dr. Taylor Jones' letter that he and Mr. Nagaoka contemplate a discussion of the *magnitude* of the magnetic stress really existing in iron under, I hope, natural conditions. A complete discussion of the sort would, I think, be of much value. It seems to me somewhat doubtful what is the true nature of the assumptions latent in Maxwell's work, vol. *ii*. arts. 630–646. I am uncertain whether his conclusions are strictly applicable to any case other than that of an infinite homogeneous medium in which the permeability is unity. In the ordinary case of a magnetic bar, some lines of force traverse the surrounding air, and complications also arise from the "free" magnetism at the ends. When, as in previous experiments on the magnitude of the stress, a magnetic bar or ellipsoid is cut in two, the stress measured is presumably that exerted across a minute air-film separating the opposed surfaces. Under such circumstances the formula $B^2/8\pi$ is, according to Dr. Taylor Jones' own experiments (*Phil. Mag.* for 1895, p. 254), a close approximation to the stress when the permeability of the bar is large.

Prof. Ewing's letter also calls for some comment at my hands. The view I advocated is hardly that he attributes to me. I expressed no opinion as to the correctness of the measure $B^2/8\pi$, assumed by Dr. More and others, for the *intensity* of the magnetic stress. Neither did I say the stress system reduced to a *simple* longitudinal tension. It is, I believe, in general considerably more complicated, and is accompanied by strain perpendicular to, as well as along the lines of force. Avoiding complications unessential to the point at issue, I simply supported the view that the magnetic stress along the lines of force is a tension, and that the associated strain in the metal along this direction is an extension.

Again, Prof. Ewing says that the case illustrated by me in p. 270 is a "special one." The illustration in question is, however, just as applicable to the case of an endless ring, to which he specially refers, as to that of a long rod. It is really based on three hypotheses or assumptions, as valid in one case as in the other: (1) that the magnetisation is uniform; (2) that the air gaps, real or imaginary, are narrow compared to the length of the element; (3) that the existence of an indefinitely thin air gap, such as may be found between two attracting pole faces in contact, does not affect the *sign* of the stress.

Now (1) is merely a definition, and (2) a mathematical expedient to simplify the proof, so that (3) alone remains. As regards (3), I merely followed Prof. Ewing in § 145 of his "Magnetic Induction," except in so far as I did not assume the existence of a narrow gap to be wholly immaterial. As Prof. Ewing admits that the stress in the case of my illustration is a *tension*, we should, if he adhered to his original views regarding (3), be now on the same side. He has, however, apparently completely altered his views, for he says: "Make the iron continuous by closing up the gaps, and the tensile stress disappears." This seems an explicit declaration that there is no such thing as a "Maxwell stress," and that consequently neither extension nor compression can be assigned to such a cause. Prof. Ewing's new view implies a sudden large discontinuity between the state of close juxtaposition of magnetic material, when the stress he admits is large, and that of absolute continuity, when the stress he says is nil.

It would, I think, be premature to do more than call attention to this apparent discontinuity until the arguments in favour of the new view are produced.

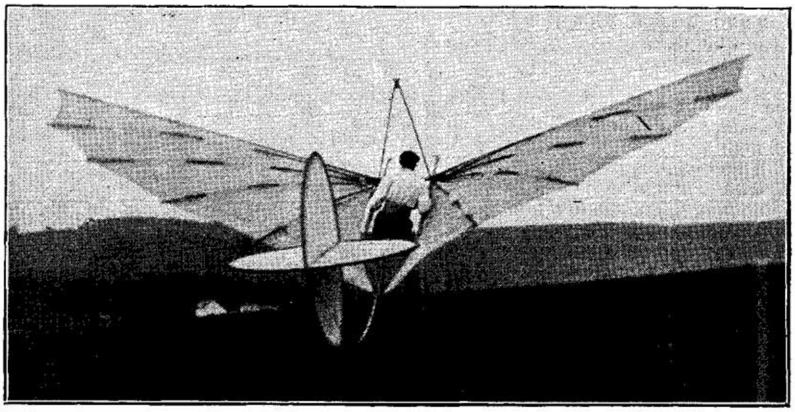
Prof. Ewing's remarks about the pulling and pushing of magnetised rings, indiarubber bands, &c., suggest that he experiences a difficulty in imagining any complete, homogeneous, isolated ring under conditions such that the normal stresses over all cross-sections are tensions. The consideration of the elastic stress system produced in an anchor ring by rotation about a perpendicular to the plane of its circular axis (line of centres of cross-sections) through the centre will be found, I think, to aid such a conception.

I must decline, I fear, to follow Prof. Ewing into the clothes-basket he refers to, though doubtless the experiment must be a fascinating one—for spectators. C. CHREE.

Kew Observatory, February 11.

Experiments with Soaring Machines.

AT the end of the article on Herr Lilienthal's soaring experiments, in your issue of January 30, reference is made to my trials, with somewhat similar apparatus, at Cardross, and it is said that so far I have not been very successful.



I do not consider this to be the case. I think that, so far as I have gone, my work has been rewarded with as much success as could possibly have been hoped for.

I only commenced to build my first machine just a year ago, and being very busy with other things, was not able to take the machine out to practise till June.

At first my wings were, as is stated, very much raised, so that the machine should have transverse stability of its own in flight, just as a kite is stable sideways when it is of this shape, or as a sheet of paper folded to a V-shape will always fall on the folded edge.

There is, however, a great practical difficulty with the raised wings, and that is, that if one stands not exactly head to wind, the machine tends to "pay off" from the wind, which then comes more on one side, and rolls the machine over.

With the wing tips lowered more as a gull's wings are, if the wind (virtual or real) is not ahead, through a squall or any other cause, the machine has very little tendency to raise on the windward side, and so be turned over by the wind.

Not appreciating this, certainly caused me some delay and breakages; but when I realised how great is the objection to the raised wings in such a squally place as Cardross, I curved the wings as shown in the second photo which I enclose, and with this original apparatus have been picked up by a puff of wind over and over again, sometimes as much as 12 feet above the spot I was lifted from, and put down again on exactly the same place. At other times, when there has not been so much wind, I ran to meet the wind with the front of the wings depressed somewhat, so that my weight is only partially taken by the wings, and in this way I am, strange as it may appear, able to run very

---

[1] "Electrostatics and Magnetism," § 290 (Roy. Inst. Friday Evening Lecture, May 18, 1860); "Electrostatics and Magnetism," § 546 (*Proc.* Roy. Soc., May 10, 1856).

[2] "Electrostatics and Magnetism," § 573.

© 1896 Nature Publishing Group

much faster than without the wings; then raising the front edge a little, I am able to take a long soar down a slight incline. The only slopes on which I was able to practise, were not steep enough to make it possible for me to soar for any great distance; and therefore I have at times, on days when the wind was fairly steady, attached a string to the front of the machine, with which a boy has run and kept me in the air for about half a minute. I never used a line when there was enough wind to pick me up without forward movement, on account of the strong winds at Cardross being so very squally.

I had to stop experimenting at the end of September; but when I left off, I was pretty sure of my balance in the air, and was able to land without damage and without falling, even when soaring over the ground very fast. And this was the whole object of my work, so that I should be pretty sure of my balance before putting motive power into the machine to make horizontal flight possible. This I hope to do this year, with a petroleum engine, either working the screw propeller in the front of the machine, or two screws, one placed under each wing, so that the machine will then practically become a Maxim machine of the smallest size possible for one man to fly with.

Mr. Maxim has shown most clearly that his large machine at Bexley will rise on the underneath side of rails put down to prevent its rising further; and what is possible for this large machine, is just as possible for a small one. And I am confident that I could maintain horizontal flight with wings similar to those I have been using, or, better still, similar to those I am now making, which vary chiefly only in mechanical detail from the first, if driven forward as I propose.

Herr Lilienthal, who most kindly showed me his apparatus, and let me see him practise last April, has kept me informed of what he has been doing.

Mr. Lawrence Hargrave, of New South Wales, kindly wrote to me suggesting that I should use double surfaces, and advocated the construction he uses in his cellular kites, as being simple and light.

Although with the double surfaces now used by Herr Lilienthal a machine with the same area of sail can be made of less extreme dimensions, and a considerable saving can probably be made in weight, it has the disadvantage of having less area to act as a parachute in the event of speed through the air being lost, either by a sudden lull in the wind, or through want of skill on the part of the flyer. And I am as yet not at all sure that the upper surface with a puffy wind would not, from the very fact of its high position, prove a source of danger rather than the reverse.

My reason for saying this is because I tried a machine at Cardross with the wings just above my head, but found the machine with the low wings very much more easy to handle, especially when the wind was puffy; but in order to make more sure of this, I shall probably make a double surface machine at once.

It is quite possible that what is best at Herr Lilienthal's ideal practising place, where he has his cone-shaped hill and flat country for miles round, so that his wind is unbroken and steady, is not best in the proverbially squally district of the Clyde, where I have had to experiment.

The accompanying illustration will probably make my letter more clear.

PERCY S. PILCHER.

Science and Morals.

A HABIT has been growing of recent years among certain scientific men, which many of those with whom I have discussed the subject join with me in regretting. It is this:—After the announcement of an interesting discovery, a number of persons at once proceed to make further experiments, and *to publish their results*. To me it appears fair and courteous, before publication, to request the permission of the original discoverer, who probably has not merely thought of making identical experiments, but who has in many instances made them already, and has deferred publication until some grounds exist for definite conclusions.

The late President of the Chemical Society, Dr. Armstrong, has sought to justify such conduct. On p. 225 of the *Proceedings* of that Society for 1894, these words are reported as having been used by him:—"After having been told so much, chemists could not be expected to remain under the imputation that they had been eyeless for a whole century, and they would undoubtedly inquire into the matter. Although no one would seek to take the discovery out of the hands of those who had announced it, chemists unquestionably had the right, not only to exercise entire freedom of judgment, but also to critically examine the statements which had been made."

These words related to the discovery of argon by Lord Rayleigh and myself; and, as we were otherwise occupied, no notice was taken of them. Events, I think, have justified the course which we then took. But now that all personal element has been removed, I feel free to raise the question—Is this recommendation precisely consistent with the highest view of scientific morality.

An analogy will perhaps help. If a patent has been secured for some invention capable of yielding profit, and some person repeats the process, making profit by his action, an injunction is applied for and is often granted. Here the profit of the business may be taken as the equivalent of the credit for the scientific work completed; no original idea, undeveloped, is of much value; before it produces fruit, much work must be expended; and it is precisely after the publication of the original idea, that sufficient time should be allowed to elapse, so as to give the author time to develop his idea, and present it in a logical and convincing form.

Should such trespassing on newly-sown ground come to be the rule, instead of, as I fervently hope, the exception, the result will be this:—Scientific men will provide their private laboratories with a good lock; they will communicate their ideas to none, until they are worked out; and the pleasant and friendly intercourse, which is now universal, must be abandoned. Such a state of matters would be greatly to be regretted; and it is obvious that the progress of scientific discovery would be not immaterially hindered, if every scientific man were obliged to protect himself against what, after all, comes near to a breach of the Eighth Commandment. WILLIAM RAMSAY.

University College, February 14.

The Former Northward Extension of the Antarctic Continent.

MR. BEDDARD, in NATURE for December 12 (p. 129) has called attention to a new fact, "loading still further the already over-weighted scale which now dips so deeply in favour of the Antarctic continent." Permit me to add another fact bearing on the question, but whose significance has been quite overlooked hitherto.

The most characteristic of types which occur in the cold and temperate fresh waters of the southern hemisphere is the genus *Galaxias*—a type whose representatives are popularly known as trout in New Zealand, Tasmania and Australia. Species nearly related (in one case claimed to be identical) are found in South America, and furnish the most cogent testimony in favour of a former connection of the several now isolated areas. None have been found elsewhere, and none were looked for from Africa; but in 1894 a species was described by Dr. Steindachner (*Sitzurgsb. k. Akad. Wiss. Wien*, ciii., abth. 1, p. 460, pl. 3, f. 2) as *Galaxias capensis*, and there is no apparent reason to doubt that the generic allocation is correct. The geographical range of *Galaxias* is then somewhat analogous to the worm genus *Acanthrodilus*, which is the theme of Mr. Beddard's note. His remarks are applicable with even greater force to the fish genus. "It is clear that, if the former northward extension of the Antarctic continent is not believed, some explanation of these remarkable facts is much wanted; on that hypothesis they are perfectly explicable."

Lest some may think the argument in question is invalidated by the so-called *Galaxias indicus* of Day, I may add that I do not think that fish has any relation to the genus to which it was referred. THEO. GILL.

Washington, January 24.

Children's Drawings.

THERE are two little boys in my circle (nephews, in separate families), who at an early stage in their attempts at drawing, have drawn things *upside down*. Thus a locomotive would be drawn with funnel pointing downwards, and wheels at the top of the figure. Has this peculiarity been noticed before, and is it common? It seems to me interesting in relation to the subject of erect vision, for the retinal image is, of course, *inverted*.

© 1896 Nature Publishing Group